areas, easterly, have a certain regularity, it is soon learned to tell nearly *when* the effect of any area noticed as approaching will commence to be felt. The instrumental signs of the weather case will show the changes commencing, and it may be judged whether or when the weather conditions noticed in the approaching area are likely to prevail. Some elementary knowledge of meteorology is needed; but much can be done by a careful watching, solely. The instruments of the weather case will show the changes anticipated when they begin to be felt and before they have arrived in their full force.

If the area noticed has been large or small, with weather conditions decided or variable, it can, from these facts, be judged often if the "coming weather," of whatever kind it may be, will be steady for a day or for days, or more rapidly changing. The case makes practicable other and many studies.

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Washington, D. C., July 21, 1878

ARE THE "ELEMENTS" ELEMENTARY?[1]

II.

IT would be a curious speculation were one to ask one's self what *is* the atomic weight of ozone? Is it 24? Is its atomic formula O1·5? or has oxygen the atomic weight of 32 and ozone of 48, and are the molecular weights 64 and 96 respectively? This can scarcely be, for the smallest amount of oxygen in two volumes of a gaseous compound of that element is certainly not 32, but 16 parts by weight. In fact *atoms* of allotropes scarcely appear to exist, the molecule appears to be the smallest amount of one of these substances that can exist either in, or out of combination. But can elements really exist in combination under various allotropic forms? We do not know. Weber thinks they can, Clarke thinks they cannot. The example of graphitic acid sometimes cited as proof of the existence in the combined form of allotropic carbon does not appear to me to *prove* either one view or the other. Graphite, may we not say, is an intermediate stage in the formation of graphitic acid from carbon? But it does not follow that the carbon in that acid is in a form different from that under which it exists, say in sugar. But it is exceedingly difficult, as yet, to attach a definite meaning to such a statement as "Carbon exists *under different forms* in this and in that compound."

The specific heats of allotropes vary. Weber has most carefully determined the specific heats of the modifications of carbon and boron. The numbers obtained at low temperatures are different, but when we come to those temperatures at which optical differences disappear, we find that differences in specific heats disappear also. At high temperatures there is but one specific heat for carbon and but one for boron; at low temperatures there are two or more specific heats for each. This seems to mean that at sufficiently high temperatures there is but one form of carbon and but one form of boron. As we do not know the atomic or molecular weights of the allotropic modifications of these elements, we can, it seems to me, draw no conclusions of any value concerning the atomic heats of these allotropes, and therefore the fact that the atomic heat of the elements is a constant number may be explained equally well on the hypothesis that the elements are all allotropes of one of themselves, or of an unknown substance, and on the hypothesis that the elements are essentially distinct forms of matter. It is well to bear in mind that, so far as our knowledge of allotropy goes—and it goes but a very little way—we have reason to believe that each allotrope has a different molecular, and therefore, probably, a different atomic weight, from every other allotrope of the same element; and, further, that we know that allotropes are at high temperatures resolved into one and the same form. Phosphorus has an abnormal vapour density: two volumes of the vapour of this body contain four (relative to hydrogen as two) atoms; red phosphorus (P_β) changes into common phosphorus (P_α) at comparatively low temperatures, therefore we do not know the vapour density of P_β. At a point not far from its boiling point, two volumes of the vapour of sulphur contain six atoms (if $S = 32$); at a higher temperature two volumes contain two atoms. We appear to have here a real gaseous allotrope. Is it analogous with ozone? Is it not therefore probable that the densities of P_α and P_β would be found to be different, supposing they were both obtained as vapours? But even P_α is abnormal. May it not be, then, that we have not as yet obtained normal phosphorus at all? That what we call phosphorus is an allotrope of the true phosphorus, viz., P_2? May it not be that at very high temperatures P_α splits up and yields true normal phosphorous vapour? Now let us briefly glance at isomers, or compounds having the same composition, but with different molecular weights. The mere fact that compounds of the same composition, but different molecular weights, exist, especially when taken in conjunction with the further fact that different compounds having the same composition and the *same* molecular weights also exist, renders the theory that the elements are really compound bodies not altogether improbable. So little has been done in the way of exact determinations of the specific heats, specific volumes, and other physical constants, of isomeric bodies, that I forbear from pressing the facts that are known into the argument, but content myself with saying that a more or less simple relation appears to exist between the physical and the chemical nature of the various isomers. The generally accepted theory of isomerism seeks to account for the facts by supposing that the atoms of isomers having equal molecular weights are differently arranged: another theory—to me it appears that the one theory is complementary of the other—supposes that the differences in the action of isomers are to be traced to differences in the amounts of "energy" possessed by these isomers (would it not be better to say, to differences in the relations between the potential and kinetic energy of isomers, and also perhaps to differences in total energy?); whichever theory is applied to isomers may be equally applied to the elements, on the assumption that these bodies are really compounds of one simple form of matter.

The positive evidence in favour of the theory of the non-elementary nature of the so-called elements is not very great. Yet, to say that the elements *are* truly elementary is, I am persuaded, a statement which is not justified by the facts which we possess. Either hypothesis may be adopted as a working hypothesis: the former, that the elements are *not* elementary, is, it seems to me, likely to lead to more discoveries, and to pave the way to more far-reaching generalisations than the latter.

But why should no one have succeeded in decomposing one of the so-called elements? In a sense we have succeeded. Ozone is an element, but it can be decomposed. Oxygen may, I think, be said to be a simpler form of matter than ozone. The introduction of the battery into chemistry led to the decomposition of potash and soda; the introduction of new engines of research may lead to the decomposition of some of those bodies, our conclusions regarding the elementary nature of which rests upon the same kind of evidence as did the conclusions regarding the elementary nature of potash and soda before the experiments of Sir Humphrey Davy. Analogy prompts us to ask, Is it not possible that what we cannot accomplish in our earthly laboratories may be actually brought about in the great natural laboratories of the sun or stars? And facts recently observed by Lockyer give some countenance to those who would answer this question in the affirmative. The spectra of compounds are as a rule more complex than those of elements. The former bodies, speaking broadly, yield channelled or band spectra, the latter line spectra. The non-metallic elements, also, yield spectra which may be generally described as channelled or band spectra, while the spectra of the metals are more to be described as line spectra. Again, with increase of temperature band tend to change into line spectra; with increase of temperature compounds tend to be decomposed into their constituent elements. But the solar spectrum is a line spectrum, the spectra of certain stars—Sirius, &c.,—are line spectra, but simpler in their character than that of the sun; the hydrogen line is predominant in the Sirian spectrum. The spectra of certain other stars—*e.g.*, the red stars—are very complicated, and consist for the most part of bands and channelled spaces.

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[1] A paper read before the Owens College Chemical Society. Continued from p. 593.

Putting these facts together, the hypothesis has suggested itself to Lockyer that the atmosphere of the red stars contains certain compounds and many non-metallic elements, that the atmosphere of the sun is characterised by the presence of metals, there the non-metals are decomposed into simpler forms, that those elementary bodies which are not found in the hotter part of the sun's atmosphere *are being* formed in the upper and cooler portions, but as they descend they are again dissociated, and lastly, that in the very hot stars our elements are for the most part resolved into simpler forms. Hydrogen, however, appears to exist there in the form in which it is known on this earth. This hypothesis is put forward tentatively by its author, and must only be accepted as a working hypothesis. It is most interesting to the chemist; in some of its bearings it also tends to throw light upon the physical conditions of the existence of stars and suns. Yet it is only an hypothesis, we must beware of accepting it as a dogma.

There is one point as bearing on Lockyer's hypothesis to which I should wish to direct attention. Non-metals show more variations in their spectra than are exhibited by the spectra of the metals. Metals yield, as a rule, only line spectra, non-metals channelled, then line spectra, as the temperature increases. Analogous with these changes in the spectra of non-metals is the well-known "plasticity" of these bodies. The instances of allotropy about which we have any accurate knowledge are instances among the non-metallic elements; probably, then, these bodies will be more readily decomposed than the metallic elements.

Putting together all that is known on the subject, the balance of probability appears to me to be in favour of the hypothesis that the elements are not really elementary. But if one is asked to put forward a positive hypothesis, not merely to favour a negative one, the task becomes much harder.

In a short, but exceedingly suggestive paper ("Speculative Ideas respecting the Constitution of Matter," *Phil. Mag.*, February, 1864), Graham tentatively put forward the hypothesis of original matter having a molecular or atomic structure, all the molecules being uniform in size and in shape, but not all possessed of the same amount of motion. In the differences in the motions of the parts of this original matter Graham sees the origin of all differences in the properties of our various elements. The gaseous molecules which we are accustomed to measure are not, says Graham, to be regarded as the ultimate molecules of the original matter, but as composed of a "group or system" of these. This hypothesis of Graham appears to me to be one of great merit. But if we start with one matter, whose molecules are of equal mass, may we not imagine these molecules originally possessed of equal amounts of motion? Having got these molecules, it is not, I think, beyond the powers of the scientific imagination to regard some of them as coming within the sphere of each others' action, and as coalescing to form new compound molecules, the mass of such new molecules being of course different from that of the original molecules. After such an encounter the new molecule will possess an amount of energy different from that possessed by the original molecules; hence it will exhibit new properties. The original matter has thus become differentiated; we have now more than a grained structure, the grains vary in mass, and in the amounts of energy which they possess. This process of evolution of higher and higher orders of molecules may proceed (may be now proceeding) until we arrive at those systems which are at present generally regarded by chemists as the molecules of distinct forms of matter, as the elementary bodies of to-day. These elementary bodies are again ready, under proper conditions, to form yet higher orders of molecules; these are our compounds, but these higher orders are less stable, under average conditions, than the lower (elementary) orders of molecules. If by any means a very large amount of energy be added to our elementary molecules they would tend to dissociate and to reform the simpler orders or groups from whence they have been derived. Such addition of energy appears to be given in the intensely hot atmosphere of the sun, where metallic bodies may seemingly remain in company with heated oxygen, yet unoxidised. If, however, a small amount of energy only be given to an element, then that element becomes ready to unite with another, or with others, to form a compound body. The bodies which we call elements would, on this hypothesis, be but intermediate stages in the evolution of complicated compounds from one original form of matter. At certain stages in this process points of comparative rest are reached; one of these points marks the existence of our so-called elements. Bodies which are elementary in our laboratories are compounds in the more energetic laboratories of the sun and hot stars. Many of our compounds, again, are elementary in the cold, listless atmosphere of the moon. Just as it is very difficult, if not impossible, to define a chemical compound, to say where the mere mechanical mixture or aggregation ends and the true chemical compound begins, so, in this view, would it be impossible to define a chemical element. Whether a substance is compound or elementary depends upon the point of time at which the investigation is made and upon the conditions of the environment. Graham has pointed out that the "colloidal state" seems to intervene between the liquid and crystalline states; the experiments of Faraday, of Caignard de la Tour, of Andrews, and more recently of Pictet and Cailletet, have taught us that between the gaseous and liquid states there is no hard line of demarcation; many facts in chemistry and in chemical physics appear to be explicable only on the supposition that the passage from mechanical mixture to chemical union is a gradual and continuous, not an abrupt and discontinuous one. Why, then, should not the passage from the one original element to the one final compound be also a gradual passage?

And as in animate nature we know that the (comparative) permanence of a species is in no way contradictory to the general law of gradual development, so in the history of molecular arrangements it may be that the present permanence of our so-called elements only marks a resting point in the slow but sure process of formation of more and more complicated compounds. The average conditions of our present surroundings may not allow of the existence of any less complicated molecular aggregations than those which we call elementary, just as they do not appear to allow of the existence of any extremely complicated aggregations of chemically united molecules.

There is another aspect of the question upon which one word may be said. I think that I am not in error in saying that the minds of most persons are imbued, more or less deeply, with the idea that nature is ultimately very simple; that could we but grasp the great laws of nature we should find them extremely simple, although the results of their actions are so wonderfully complex. This belief appears to be deep-rooted in most minds, yet if we are to study nature aright we must, I am persuaded, set it aside. We must be content to take nature as she is, *i.e.*, we must do our best to amass facts, and from these we must draw the conclusions warranted by the facts. Now as our knowledge of nature extends do we find that she becomes to us more and more simple? Yes, and No. It has been now and again given to a gifted few to pierce through the maze of tangled facts and to espy the great principle which binds them into an harmonious whole. But even in the case of these great generalisations, exact experiment and observation frequently show that little details have been overlooked—that the great simple law is too simple—that there are discrepancies, *very small*, it is true, but still there they are, demanding an explanation, telling us that our law does not express the whole of Nature's facts. Nature appears to be truly infinite; and it is well to remember that, *we can never get sensibly nearer a knowledge of an infinity.*

This idea of the simplicity of nature is very apt to lead us to adopt the hypothesis of the non-elementary nature of the elements without sufficient evidence. The idea that all the elements are really compounds of one primary form of matter is a most fascinating idea, it *seems* to be so much in keeping with the simplicity of nature; it is so symmetrical, it surely *must* be true. This is just how the old alchemists reasoned; we must absolutely forbid these *à priori* conclusions to influence us as students of nature. The hypothesis of the compound nature of the elements, of the existence of but one, or even of a few primary forms of nature, fits in with the nebular hypothesis of the formation of the worlds, but have we facts to support it? If one can only come back to facts we need not fear to start what may appear to be wild and romantic theories.

The outcome of the whole matter is this:—we want more knowledge, our facts are few and vague; there is room for almost unlimited work. Ask Nature; trust her: be sceptical of your own interpretations of her answers.

M. M. PATTISON MUIR

THE LATE SIR RICHARD GRIFFITH, BART.

WE have just laid to rest all that is mortal of the "Father of Irish Geology" in Mount Jerome Cemetery, at the ripe age of ninety-four years. Few public men in Ireland have done so much for the material advancement of their country. If "the age makes the man" the late Sir Richard Griffith was the man whom the age called forth to indicate the road to material improvement at a time when roads, railroads, drainage works, and similar agents were urgently required in this country. Griffith's geological knowledge was the basis of his power; and while few understood, or cared to understand, the principles by which his judgment was guided, Government and the public were always ready to put faith in their application. Amongst the useful works carried out under his direction were the roads which he constructed or improved in the counties of Cork, Kerry, and Limerick, during the time when the Marquis Wellesley was Lord-Lieutenant. Some of these roads are striking examples of engineering skill. I have recently travelled on one of them, namely, that which crosses the wild and rocky range between Kenmare and Glengarriff. Before this road was made the country was inaccessible and the haunt of Whiteboys; now no district in the British Isles is safer for the traveller, and, I may add, more full of bold and beautiful scenery. With reference to Griffith's services to the cause of Irish geology, it is unnecessary for me to say a word here, except in so far as regards the public department with which I have the honour to be connected. It is to this subject that I wish especially to direct the attention of the readers of NATURE, as I am anxious to pay a tribute to the remarkable acumen which Griffith exhibited in determining the age of the various formations which are to be found in this country, as exemplified in at least one special instance.

It is well known that there is one point in the geological structure of the south-west of Ireland on which there has been a difference of opinion between the Government geological surveyors and Sir R. Griffith; I refer to the age of that great group of rocks which occupies the mountainous districts of the Dingle promontory, and those of Killarney, the Reeks, and Glengarriff. These were called by the late Prof. Jukes "The Dingle Beds," and they consist of a series of purple slates traversed by cleavage planes and massive green grits, and thrown into numerous grand flexures. They are of great but unknown thickness, as in the Dingle promontory they are overlaid unconformably by the beds of the Old Red Sandstone. In the Dingle promontory these beds are seen in contact with fossiliferous beds of recognised Upper Silurian age, and the whole series had been referred by Griffith to the "Silurian" formation, as may be seen by reference to his geological map of Ireland (edit. 1855). In a similar manner the mountainous regions abovenamed, and lying to the south of Dingle Bay, were mapped and coloured as "Silurian," and were separated off from the Old Red Sandstone throughout the counties of Cork, Kerry, and Waterford.

The views thus held and published by Griffith with regard to the geological age of the rocks forming the south-western highlands were not upheld by the officers of the Geological Survey, who, we may be sure, spared no pains to come to some clear decision on the question. On the maps of the Survey the mountains of Kerry and Cork are coloured "Old Red Sandstone," and "the Dingle Beds," with a distinct colouring, are placed in a position intermediate between the Old Red Sandstone and that of the Upper Silurian. Prof. Jukes, in the "Explanations" to accompany the maps of the Survey, has fully entered into the reasons which induced him and his able colleagues to arrive at this decision. Certain apparent obscurities in the sections of the Dingle district and those of the neighbouring regions prevented them accepting Griffiths' views, and the whole matter was left an open question, subject to further investigation.

Under these circumstances—the time being favourable—I received the sanction of the Director-General to make a preliminary examination of the sections at Dingle and in the districts of Killarney, Kenmare, and Glengariff—with a view (if possible) of coming to some decision on a question which has been confessedly left in an unsatisfactory position. In this tour I was accompanied by Mr. J. O'Kelly and Mr. A. McHenry, officers of the Survey—and we have returned from it fully satisfied in our own minds of the correctness of Sir R. Griffith's views regarding the age of the beds of the Dingle, Killarney, and Glengarriff Ranges. To our minds the evidence is clear and satisfactory that these beds are really of Upper Silurian age, as maintained by Griffith. Into this evidence I cannot enter here, but hope to do so at some length in another place. It was with great gratification that some days since I addressed a letter from Eccles' Hotel, Glengarriff, to Sir R. Griffith, announcing the result of our investigations. I little knew that at that moment the spirit of our venerable friend had passed away! Few men were less dogmatic in maintaining their conclusions than Griffith. If others differed from him he remitted the matter to the arbitrament of time, satisfied that if he was in the right time would show it. In *this* case it is only justice to his